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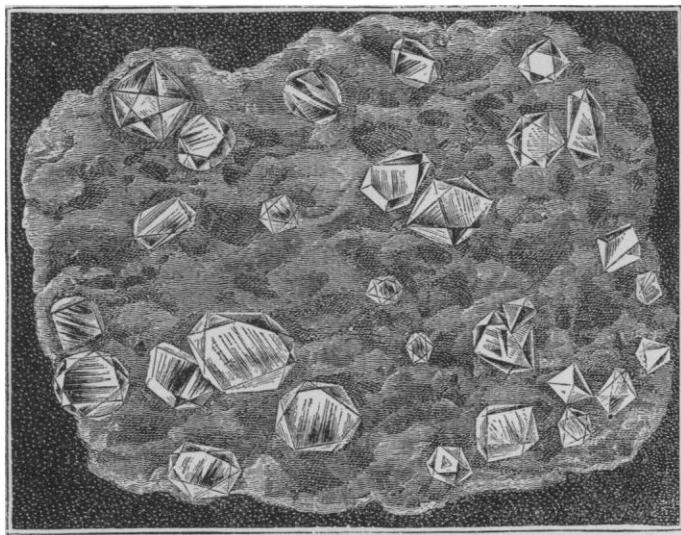
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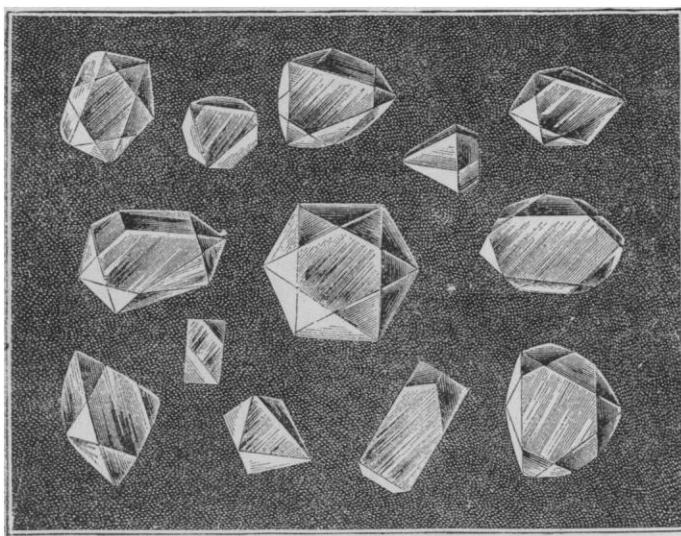
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pend principally upon the fire, which regulates and varies the chemical action. The crystals obtained in 1877 were laminated and friable. They were very thin, and embedded in a vitreous mass, which rendered it almost impossible to isolate them. Besides this, their chemical composition varied to a certain extent. By the new process they are easily separated from the porous matrix in which they are formed. The matrix is thrown into water, which is violently agitated. While the light matrix is broken and remains suspended, the rubies settle down on the bottom of the glass. They



are very clean, and it was found unnecessary to apply any acids for further cleansing. They are rhombohedral and exactly like natural rubies. Numerous analyses showed that they did not retain a trace of baryte, and that they were formed by pure aluminum colored by traces of chrome. The crystals are regular and of adamantine lustre. They are of perfect transparency, as hard as natural rubies, and cut topaz. Like the natural rubies, they turn black on being heated, but resume their color after getting cold



again. Having thus produced by synthesis rhombohedral crystals of rubies with all the physical and chemical properties of the most beautiful natural rubies, and forming them in a matrix which may be compared to that enclosing the natural mineral, Fremy and Verneuil believe they have definitely settled the question of the origin of rubies. So far, the experiments have been made with 50 grams of material only, and the crystals have therefore been comparatively small, not exceeding 0.02 of an inch in diameter. The authors, however, propose to continue their experiments on a larger scale, and expect to be able to make rubies of large dimensions.

LETTERS TO THE EDITOR.

** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

Is the Rainfall increasing on the Plains?

IN connection with the recent discussion of the question of increasing rainfall west of the Mississippi River, I wish to call attention to some serious errors in the rainfall record at Fort Leavenworth,—a record fifty years in length, and therefore frequently quoted in support of the popular view. While examining these observations, I recently found that the precipitation for January, 1871, was given as 11.25 inches,—a most extraordinary amount. Suspecting such a result, I examined the files of the *Leavenworth Times*, and found that the precipitation for that month, as measured by Mr. F. Hawn, was 0.14 of an inch of rain, and 9.25 inches of unmelted snow. Unquestionably the Fort Leavenworth record is also mainly composed of unmelted snow. Further examination showed that the amounts recorded for the other winter months of 1871 and 1872 contained a similar error, and that consequently the total for 1872 should be 41.6 instead of 51.6, and for 1871 should probably not be greater than 35.5 instead of 56.75.

These latter values for the totals of those years are given in the 'Smithsonian Tables' and in the 'Reports of the Kansas Board of Agriculture,' and, so far as I know, have never been corrected by any one that has used these observations in discussing the question of a climatic change in rainfall.

If such errors as these exist in the records, it is not surprising to find that the rainfall of Kansas is increasing.

GEORGE E. CURTIS.

Topeka, Kan., April 10.

Scarlet-Fever.

I WOULD call attention to the fact that in many of the scarlet-fever reports published in your columns an assumption has crept in which seriously injures the value of the conclusions thus based.

All disease has a *first* case in any locality: *this* is the case only of real use to investigate scientifically. Other subsequent cases may or may not be due to the same cause as the first, or to contagion. To assume that a case, however closely following a first case, is due to contagion or infection from it, not allowing ample margin for other as yet unknown causes, is simply stupid, as it weakens arguments in a good cause and for the public good.

I had this winter a boy with his second genuine attack of scarlet-fever within six months. No cause of either attack was found. His brother and sisters did not suffer from contact with him, although it was attempted, of course, to isolate the patient. I myself caught the disease at about this time, but I am by no means willing to admit a belief that such disease came to me from contact with this or other patient. Many cases are known to me where exposure wholly failed to cause this disease, even in weak, poorly nourished individuals.

If any time is more dangerous than another in regard to liability to cause spreading of the disease, it would not be, according to my experience, that of the much talked and written of period of desquamation.

JOHN DIXWELL, M.D.

Boston, Mass., April 16.

Queries.

31. BLONDE AND BRUNETTE.—What is a blonde, and what is a brunette, and what is she who is neither of these? Definitions of the words I can find in a dictionary: they do not cover the ground. A woman with black or dark brown hair and eyes and a dark complexion is a brunette. But here is one with those eyes and hair and a very light complexion: she is not a pure brunette; what is she? A girl with light hair and eyes and a dark complexion is not a blonde; what is the name for her? What is she whose hair is almost black, complexion dark, but light-gray eyes? (By 'complexion' is meant the color of the skin of the face.)